Reviewer’s report

**Title:** Psychosocial risk factors for hospital readmission in COPD patients on early discharge schemes: a cohort study

**Version:** 1  **Date:** 20 April 2011

**Reviewer:** Kim Lavoie

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**Summary**

This study assessed the impact of various psychosocial risk factors (anxiety, depression, social support, social deprivation) on risk for readmission for acute exacerbation in 79 patients with COPD referred to early discharge schemes (EDS). Main results were that depression predicted readmission after adjustment for covariates; home ownership was associated with the total number of readmissions; and that compared to non-readmitted patients, those who were readmitted had worse quality of life scores at 12 months. The authors concluded that both depressive symptoms and SES (home ownership) predict readmissions for COPD in patients referred to EDS, and that future work should focus on interventions designed to target these factors to optimize care of patients discharged to EDS.

**Study strengths**

Overall, this was an interesting study conducted on a very important topic: the impact of psychological and social factors on COPD exacerbation risk. Previous work has shown that COPD is associated with significant psychological/psychiatric comorbidity (more than many other major chronic illnesses including CVD and asthma), and disproportionately affects lower SES groups who tend to demonstrate higher rates of smoking (the major cause of COPD). Major strengths of this study include examining a range of both psychological (depression, anxiety) and social (social support, social deprivation) factors in relation to exacerbation risk; the use of well established and well validated instruments and measures (HADS, ESSI, SGRQ, CCI, Carstairs scores); the use of a prospective design which allows for examination of the temporal (rather than cross-sectional) associations between psychosocial factors and exacerbation risk; a well-defined sample (i.e., COPD diagnoses were objectively confirmed); care to adjust for important covariates (age, sex, lung function, comorbidity, previous COPD admission); and a reasonable retention of participants over 90 day (85%) and 12-month (78%) follow-ups.

**Study limitations/Major Compulsory Revisions**

Despite several study strengths, this study also has a number of important weaknesses that limit its potential contribution to the literature in its present form. I have detailed each of my concerns below, with the goal of providing
constructive feedback for the authors:

1. First, the authors appear to have missed some key publications in their literature review. Of note, one was a recent review article on this very topic (see Laurin et al, Therapeutic Advances in Respiratory Disease, 2010), as well as a similar original article by the same author (Laurin et al, Psychosomatic Medicine, 2009) that examined the impact of mood and anxiety disorders on exacerbation risk in COPD patients (most of which appeared to have been followed by an nurse-lead management program which seems conceptually similar, if not equivalent to, EDS. The authors are therefore encouraged to review these papers and discuss their findings in the context of a more thorough literature review.

2. The sample size in this study was very small with only 79 patients recruited over a 15 month period in three centers, and 43 eligible patients reportedly refused to participate, yielding a similarly modest participation rate of 65%. This calls the generalizability of the findings into question, which is important in the context that the sample is already unique in that all patients were discharged to EDS and may not be representative of all COPD patients. There was also no explanation for the low participation rates? I wonder about the extent to which the home visit (which followed a phone call to invite participation for patients to sign consent and obtain baseline and follow-up data) was not considered intrusive or otherwise unappealing to patients. This type of approach (home visits) has been found in the CVD literature (in a nurse-lead intervention for patients post-MI) to be very stressful for women, who relative to men were more likely to die if they received the home-based intervention than if they had been randomized to the control group (see Frasure-Smith N, MHART Study). This highlights the importance of being very careful with certain design aspects of studies, and may have been the reason why so many patients refused participation. However, it is possible the authors recorded reasons for refusal (which may have nothing to do with the potential stress of ‘having someone over’), but this information would be important to include in order to determine if reasons were systematic or random.

3. Several justified exclusion criteria were listed, yet no information is provided as to how many patients were excluded for each of these reasons.

4. Baseline data appears to have been collected in the patient’s home, but it is not clear who conducted this baseline assessment. The follow-up was conducted (again at the patient’s home) by the PI of the study, but it is not clear if the same person conducted both assessments. Similarly, it is not clear if the PI (who conducted follow-up assessments) was blind to patient’s baseline psychosocial risk factors. This needs to be clarified.

5. The primary outcome was readmission for exacerbation or death within 1 year post-index admission. However, power analyses (page 6) appear to have been conducted using SGRQ scores as the primary outcome, which does not make sense given exacerbations/death was the primary outcome. As such, as written, the sample size appears to have been determined based on an inappropriate outcome, which suggests that the study is either not adequately powered or at least, inappropriately powered, which is an important study limitation.
6. A related problem is with the statistical analysis procedure. The authors appear to have conducted three separate regression analyses: univariate logistic regression to predict readmission; multiple logistic regression to determine OR of readmission; and finally, Cox regression to determine HR and time to event (with Poisson for number of admissions over the follow-up). Of note is that this is not entirely consistent with what I thought was their primary outcome: exacerbation or death. Nonetheless, these analyses seem overly complex for a study with only 79 participants. In this reviewer’s opinion, it would have been most appropriate to conduct a Cox logistic regression (to yield HR for prospective data) with exacerbations as the primary outcome (which could have been combined with death, though death is not always due to pulmonary causes, even in patients with COPD), and an appropriate (given the sample size and expected number of exacerbation events) number of a-priori-determined covariates should have been selected, based on known of theoretical links with the primary outcome and/or predictors (e.g., age, some measure of COPD severity such as lung function or number of previous exacerbations, smoking, etc). Kaplan-Meier survival curves could then have been used to present time to first event data. The initial univariate logistic regression appears to have been conducted to determine covariates, and the authors set p at .01. Yet for all other analyses, they set it at .05. Table 1 reveals that one of the authors’ major predictor variables, HADS depression, would not have been significant had the authors set p at .05 for this analysis (which leads this reviewer to wonder if that is why they set a different p-value for this particular analysis?). Also, given that depression was not significant at p<.05 but was significant in the multivariate analysis, how do the authors explain this apparent discrepancy? Overall, the statistical analysis strategy would benefit from being re-considered and better justified, as well as the power analyses/sample size estimate justification. The fact that all three analyses appeared to yield different results suggests that this is warranted.

7. Related to the results and as mentioned above, the three regressions appeared to yield different results, which was not really addressed in the manuscript. Given such a small sample size and use of different covariates in each model, there is some concern that many of the results may have been spurious.

8. On page 9 of the discussion, the authors mention not finding any risk of death associated with depression, yet don’t mention the most plausible reason: the study was not adequately powered to examine this endpoint.

9. Also on page 9 of the discussion, the authors mention that “depression is casually associated with exacerbation frequency”…do they mean casually or causally? This needs to be clarified, but if it is the former, I am not sure the present data provide solid enough support to suggest a causal link between depression and COPD exacerbations. Moreover, given the authors’ emphasis on the depression-exacerbation risk finding, it was surprising that they did not discuss any potential (plausible) mechanisms that could explain this link.

10. It was somewhat difficult interpreting the Carstairs score, given that the authors only said that higher scores were indicative of worse deprivation. Could they provide the range of possible scores or place them in the context of other
studies? This would help in the interpretation of values presented in Table 1.

11. On page 10 of the discussion, the authors discuss the significant association between “home ownership” and total number of readmissions. However, this finding is also difficult to interpret without a clear definition of what is meant by “home ownership”. For instance, patients may be financially quite secure, yet due to poor health be forced to live in an assisted living facility or live with adult children. The authors also seem to interpret home ownership as a measure of SES; yet it is possible that current home ownership may also reflect autonomy (which could be affected by health status) as opposed to SES. Further clarification of this measure and how it should be interpreted is warranted.

12. Again on page 10 of the discussion (3rd paragraph), the authors discuss findings of temporal deterioration (over 12 months) of psychological and quality of life scores in patients who were readmitted vs. those who were not. What is unclear is the extent to which they are arguing that it is the psychosocial deteriorations that precede the readmissions (which seems to be the primary premise of the article) or that readmissions lead to deteriorations in psychosocial functioning. Though both are equally plausible and a bi-directional relationship can be reasonably hypothesized, it is not clear what position the authors are taking in the context of their findings. This could be clarified.

13. The authors appropriately point to the fact that their sample was highly self-selected, and that non-participants may well have been sicker or more psychologically distressed than participants (page 10). Was there any attempt or opportunity to collect even basic demographic data on refusers? This would help determine if there were in fact, any systematic biases associated with participation or non-participation.

14. The authors also point out an important limitation of their study: the failure to collect data on participation in pulmonary rehabilitation, which is part of the gold standard of treatment for COPD patients to prevent exacerbations, slow the deterioration of the disease, increase functional autonomy, and which has been shown to improve psychological distress levels. It was therefore surprising, given the focus of the study, that this data were not collected. Given that the investigators/research personnel appear to have access to patient hospital records, is it not possible to verify patient participation in PR?

15. Although the clinical implications of this study are important, and suggest a need to target psychosocial factors in COPD patients to prevent future exacerbations (this is certainly supported by previous studies), I would not necessarily limit these interventions to “nurse-lead minimal psychological interventions”. Psychiatric morbidity (ie, mood and anxiety disorders) is just as common as high levels of anxiety and depressive symptoms, and may require more specialized interventions by qualified mental health specialists (i.e., psychiatrists, clinical psychologists), particularly those that can administer the most empirically-validated treatments for mood and anxiety disorders (SSRI pharmacotherapy and cognitive-behavioural therapy). So I would perhaps extend this part of the conclusion to include these additional considerations.

Minor points/Minor Essential Revisions
1. All tables should be labeled (Table 1, Table 2 etc), unless otherwise required by the journal.

2. Reference 39 is improperly referenced (the author is Michael Babyak, so M instead of B).

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

'I declare that I have no competing interests'